A COMPARISON BETWEEN LORENTZ'S ETHER THEORY AND SPECIAL RELATIVITY IN THE LIGHT OF THE EXPERIMENTS OF TROUTON AND NOBLE

by

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(*committee signature page*)

For Suzy

I was just getting up
Hit the road before it's light
Trying to catch an hour on the sun
When I saw those thrashers rolling by
Looking more than two lanes wide
I was feeling like my day had just begun

—Neil Young

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In Part One of this dissertation, I analyze various accounts of two etherdrift experiments, the Trouton-Noble experiment and an earlier experiment by Trouton. Both aimed at detecting etherdrift with the help of a condenser in a torsion balance. I argue that the difficulties ether-theorists Lorentz and Larmor had in accounting for the negative results of these experiments stem from the fact that they did not (properly) take into account that, if we charge a moving condenser, we not only change its energy, but also its momentum and its mass. I establish two additional results. (1) The Trouton experiment can be seen as a physical realization of a thought experiment used by Einstein to argue for the inertia of energy. (2) Closely following Rohrlich, I develop an alternative to Laue's canonical relativistic account of the Trouton-Noble experiment to show that the turning couple Trouton and Noble were looking for is a purely kinematical effect in special relativity. I call this effect the *Laue effect*.

In Part Two, I use these results to illustrate some general claims about the post-1905 version of Lorentz's ether theory. I use (1) to illustrate that Lorentz needs to assume more than the contraction of rods and the retardation of clocks to make his ether theory empirically equivalent to special relativity. I use (2) to illustrate that what makes the addition of such assumptions unsatisfactory is not that it would make the theory *ad hoc*, in the sense that it would compromise its testability, but that it makes Lorentz invariance a symmetry of the dynamics in a classical Newtonian space-time, whereas, in fact, it is a symmetry of the relativistic Minkowski space-time. To provide the necessary context for my claims, I give a detailed account of the conceptual development of Lorentz's theory from 1895 to 1916. In particular, I analyze the relation between the so-called theorem of corresponding states and what I call the *generalized contraction hypothesis*. I show that the various versions of Lorentz's theory have been widely misunderstood in the literature.

Preface

This dissertation has been long in the making. I worked on this project—on and off, interrupted by more pressing concerns—for a little over a decade. It seems appropriate to begin this report of my findings with a brief retrospective. This will give the reader an impression of my motivation for pursuing the topics I will discuss, and it will give me an opportunity to thank the many people who helped me in one way or another.

I first got interested in the role of my countryman H. A. Lorentz in the history of the special theory of relativity back in 1984 when I wrote a text providing a historical introduction to special relativity for Dutch high school students. I used this text as the basis for a class for physics freshmen I taught for three consecutive years at the University of Amsterdam on Lorentz and special relativity. I am grateful to Piet Molenaar and the late Lex van der Meer for the opportunity to teach this class, and my students for their very active participation.

Trying to locate books on the history of special relativity in the library of the institute for theoretical physics at the University of Amsterdam, I discovered they were all charged out to a certain A. J. Kox, who turned out to be a world renowned Lorentz scholar. Kox kindly offered his expertise along with numerous references and photocopies of papers by Lorentz and others, thus initiating me in the field of the history of science. I ended up writing my Master's thesis in physics under his supervision. My intellectual debt to him is enormous. I sincerely hope he likes what I have to say in the following pages about the man we affectionately refer to as *HAL*. Practical problems unfortunately prevented him from serving on my doctoral committee, but he continued to offer his much appreciated help via email.

It was also during my years at the University of Amsterdam that my views on special relativity and my understanding of what makes special relativity a better theory than Lorentz's empirically equivalent ether theory were shaped decisively in a series of long conversations with Jon Dorling, philosopher of science at the University of Amsterdam. I hope to have added a few arguments to his.

Sometime in 1986, I hit upon the discrepancy between Larmor's and Lorentz's ether theoretic accounts of the Trouton-Noble experiment, the problem that gave my dissertation project its focal point. I was unable to sort out this problem at the time, but my efforts did deepen my understanding of the crucial theorem of corresponding states and the relation between that theorem and what I have come to call the generalized contraction hypothesis. In 1988 and 1989, I was invited to give talks on these results at the University of Utrecht. I

want to thank Dennis Dieks, Jan Hilgevoord, and Jos Uffink for these invitations and for stimulating discussions.

In 1988, at a conference in Luminy, I met Arthur Miller, whose 1981 book on special relativity in many ways formed the starting point for my own work. Although I strongly disagree with his interpretation of Lorentz's work at some points, his book and his papers have been a true inspiration to me, and the conversations I had with him during that conference provided more encouragement that he probably realized.

At that same conference, I met my thesis advisor, John Norton, who suggested I enroll in the History and Philosophy of Science graduate program at the University of Pittsburgh. My intellectual debt to him is of the same order of magnitude as my intellectual debt to A. J. Kox. In his class on history of special relativity in the spring of 1990 and in an independent study I did with him in the spring of 1992, I returned to the problem of the Trouton-Noble experiment and its ramifications for a comparison between special relativity and Lorentz's theory. To mention just one of his many contributions, it was John Norton who urged me to carefully look at Laue's work on the behavior of stressed bodies in motion. The importance of Laue's analysis for the argument in this thesis can hardly be overstated.

I could not have solved several technical problems in both the ether theoretic and the relativistic accounts of the Trouton-Noble experiment without the help of Tony Duncan, theoretical physicist at the University of Pittsburgh and one of the best instructors I have ever taken a class with. The dissertation not only benefited from his technical expertise (especially on issues concerning the energy-momentum tensor), but also from the sometimes heated debates we have had over some of the conceptual issues. It seems fitting that, after serving on this committee, Tony Duncan will spend a semester at Columbia University, on the same fellowship that brought Lorentz to New York in the spring of 1906.

I also want to thank the other members of my committee. I hope John Earman and Wesley Salmon will recognize some of the Pittsburgh approach to philosophy of science in the conceptual analysis in this dissertation. My views on history of science have been strongly influenced by Ted McGuire. I hope he will accept the historical relevance of my analysis. To Gordon Fleming I owe my sensitivity to hyperplane dependence, without which I would not have found the alternative to Laue's canonical relativistic account of the Trouton-Noble experiment, an alternative that plays an important role in my arguments.

In this context, I also want to thank David Sandborg for producing the diagram illustrating an argument in which I use these two accounts of the Trouton-Noble experiment.

In the summer of 1994, I gave a talk at the *Max-Planck-Institut für Bildungsforschung* in Berlin based on the prospectus for this dissertation. Peter Damerow, Yehuda Elkana,

Wolfgang Lefèvre, Jürgen Renn, and, especially, John Stachel all offered very interesting suggestions. Rather than a word of thanks, I feel I owe them an apology for not including more of these in the final product. I am looking forward to discuss my latest findings with them when I go back to Berlin this summer.

My analysis of Lorentz's theory provides a new angle on the long standing debate about whether that theory is *ad hoc*. I am grateful to Adolf Grünbaum, a key player in this debate, for many useful suggestions. I regret that I have not had a chance so far to meet with him to discuss my final conclusions on this matter.

In the final stages of my project, two fellowships freed me from my teaching duties and allowed me to work full time on my research. I received a pre-doctoral fellowship from the Andrew Mellon Foundation for the academic year 1993–1994, and the John C. Slater Fellowship for the history of 20th century physics from the American Philosophical Society for the academic year 1994–1995. I want to thank both organizations for awarding me these fellowships.

I also incurred debts of a more personal nature. First of all, I want to thank my fellow graduate students, both in Pittsburgh and in Amsterdam, for providing a very stimulating social and intellectual environment. I want to name just a few: Aristidis Arageorgis, Carl Craver, Ofer Gal, Giel Halberstadt, David Hillman, Cory Juhl, Hans Montanus, Madeline Muntersbjorn (formerly known as Madeline Larson), Keith Parsons, and Laura Ruetsche. To Jonathan Simon I want to apologize for canceling half the *Subbuteo* season on him.

I also want to thank my family (*pa en ma, Ruud en Denise, Ilse en Bert, Lieke en Stephen, ooms en tantes*), and my wonderful in-laws, all forty or so of them, but especially Basil Wilson (for helpful discussion) and my wife's twin sister Sara (for fostering my love of the theater, for instance, not to mention *Glamour Magazine* and *NBC*'s *Sisters*).

Finally, I want to thank Suzanne Durkacs, my wife of almost three years. I must have freaked out after each fifty pages I finished, and she got me back on track no less than five times. I am sure the committee will be happy she left it at that. On top of this, she offered her skills as network manager for University Relations, and deserves at least partial credit for the one thing I tend to be a perfectionist about: the layout. For years, I have been walking around with the idea of dedicating my dissertation, if I were to ever write one, to the memory of Evie-Marieke van de Wiel (1960–1987), who taught me some valuable lessons about pursuing your dreams. I am sure Evie would have understood that I want to dedicate my work to Suzy instead, who actually made a dream or two come true.

TABLE OF CONTENTS

Ch. 2: Relativistic accounts of the experiments of Trouton and Noble

Ch. 4: Comparing Lorentz's understanding of Lorentz invariance to Einstein's

LIST OF FIGURES